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M. J. JOHNSON, Esq. President, in the Chair.

Rev. F. Silver, Norton, near Market Drayton, Shropshire, was balloted for and duly elected a Fellow of the Society.

On the Measured Distance of 70 *Ophiuchi.*
By the Rev. W. R. Dawes.

"The fine double-star 70 *Ophiuchi*, being quite within the optical power of comparatively small telescopes, has not received from me that constant attention which, perhaps, from its great intrinsic interest it deserves. As, however, the elements of the orbit recently computed by Mr. E. B. Powell give a continued *increase* of distance, while the measures of several observers show that that element is *decreasing*, I am induced to anticipate in this instance the publication of the results I have obtained, forming a portion of a large mass of observations, which, I hope, will before long be in the hands of the Astronomical Society.

"My mean results are the following:—

Pos.	Obs.	Nights.	Dist.	Obs.	Nights.	Epoch.
°			″			
118·83	15	3	6·804	16	3	1848·12
114·66	29	7				1853·60
			6·489	24	5	·68
113·71	18	4	6·339	14	3	1854·73

"These measures fully support the conclusion indicated by those of Captain Jacob, Mr. Miller, and Mr. Fletcher; and, indeed, do not differ widely from any of them. The decrease of distance to the extent of about 0″·4 or 0″·5 since 1848 seems to be well established.

"*Wateringbury, May* 1, 1855.

Observation of an Occultation of Venus by the Moon. By J. Ferguson, Esq.

(*Letter to Lieut. Maury, U.S.N., Superintendent of the National Observatory, Washington, communicated by Lord Wrottesley.*)

"I submit the following results and description of the occultation of the planet *Venus* by the moon, observed with the large equatoreal on the 18th of the present month. The following are the meteorological indications for the time of observation taken from the journal of the Observatory,—

Bar. 29·912. Therm. attached, 75°. Ext. therm. 70°·5.

"The night was sultry, with a damp atmosphere, but quite serene and clear, till within 15 degrees of the horizon, below which was a brownish haze, as of Indian summer. The dark limb of the moon was distinctly seen with the naked eye until after the occultation. After 8 o'clock the planet became ill-defined, having a reddish-purple tinge. The power of the eyepiece used was 120, with which the following times were observed:—

"At $8^h\ 37^m\ 30^s$ M.T. Wash.—The limbs were in contact. This time is uncertain two seconds, owing to the flame-like edge of the planet, the moon's limb being well defined.

"At $8^h\ 39^m\ 10^s$.—The planet was seen half its diameter within the limb of the moon, exhibiting no diminution of light or of magnitude, but showing as if it were on this, and not on the other side of the moon.

"At $8^h\ 39^m\ 38^s$.—The first diminution of magnitude was apparent, the inside or cut edge being straight and well defined, the planet still showing as if projected on the surface of the moon. The last two phases were observed with great care.

"At $8^h\ 40^m\ 7^s$.—The immersion, which was sudden, but not instantaneous.

"*U.S.N. Observatory, April* 19, 1855."

Account of the Operations for determining the Longitude of Fredericton, New Brunswick, by Galvanic Signals. Extracted from a Report to the Lieutenant-Governor.

(*Communicated by the Astronomer Royal.*)

"The Government of the United States has spared neither pains nor money in determining by the most approved astronomical methods, and by interchanges of upwards of one thousand chronometers, the difference of longitude between Greenwich and Harvard College Observatory, in order that the latter might

serve as a point of reference in conducting the operations of the Coast Survey. By our telegraphic communication with Boston, and through the kind co-operation of Professor Bond and his assistants, we have, at a comparatively insignificant amount of trouble and expenditure, been enabled to avail ourselves of the labours undertaken for the above-mentioned purpose, and thus to ascertain the longitude of Fredericton with probably an equal degree of precision.

"It was originally intended to have an unbroken telegraphic communication between the Fredericton Observatory and that of Harvard University, but in consequence of the wires from the latter to the office in Boston being out of repair, Professor Bond found it necessary to trust to two excellent sidereal micrometers for the interval; and remarks, that on examination he was induced to believe that no greater error had arisen from this source than would have taken place had the communication been made from the room adjoining the transit instrument. Professor Bond's chronometers were carefully and repeatedly compared with his transit-clock and with each other, both before and after interchanging signals, so as to ascertain their error and rate; and at both observatories, on each day of operations, the meridian passages of a number of stars were observed, in order to obtain the error and rate of the transit-clocks.

"On the evening of the 23d of January, 1855, we received the first series of signals from Boston. Mr. Coolidge (Mr. Bond's assistant) commenced at an even minute by his chronometer, and sent us second-beats for fifty consecutive seconds. This was continued for ten successive minutes, beginning always at the even minute, and we carefully noted the times by our transit-clock. On examining all, we found that the times of the first signal would be as exhibited in

Table I.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	6 29 37·7	1 40·18	6 27 57·52
Cambridge	6 10 21·5	0 21·36	6 10 0·14
		Hence the difference of longitude	0 17 57·38

"On the evening of the 2d of February we took the initiative, and sent a series of signals to Boston, the result from which is given in

Table II.

	Clock Time.	Clock Error +.	True Sidereal Time.
	h m s	m s	h m s
At Fredericton	6 38 0	2 25·88	6 35 34·12
Cambridge	6 18 13·3	0 36·48	6 17 36·82
		Hence the difference of longitude	0 17 57·30

" On the same evening we sent another series, and the result deduced from them is shown in

Table III.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	6 49 0·0	2 25·88	6 46 34·12
Cambridge	6 29 13·3	0 36·48	6 28 36·82
		Hence the difference of longitude	0 17 57·30

" On the same evening we received from Cambridge a series of signals, which give a result exhibited in

Table IV.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 4 23·6	2 25·88	7 1 57·72
Cambridge	6 44 37·0	0 36·48	6 44 0·52
		Hence the difference of longitude	0 17 57·20

" On the evening of the 10th of February we were again in telegraphic communication with Boston, and the result of the first series of signals which were sent from Fredericton and recorded at Boston is exhibited in

Table V.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 1 0	3 23·7	6 57 36·3
Cambridge	6 40 27·05	0 47·89	6 39 39·16
		Hence the difference of longitude	0 17 57·14

" The second series of signals on the same evening was transmitted from Boston and recorded at Fredericton, and the result is as shown in

Table VI.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 17 21	3 23·7	7 13 57·3
Cambridge	6 56 48	0 47·89	6 56 0·11
		Hence the difference of longitude	0 17 57·19

" We next sent a series of signals to Boston, the result derivable from which is given in

Table VII.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 43 0	3 23·7	7 39 36·3
Cambridge	7 22 27	0 47·89	7 21 39·11
		Hence the difference of longitude	0 17 57·19

" We then received from Boston and recorded at Fredericton another series of signals (the fourth of the same evening), the result of which is shown in

Table VIII.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	7 51 21	3 23·7	7 47 57·3
Cambridge	7 30 48 (17·49 × 30·4 =)	0 47·89	7 30 0·11
		Hence the difference of longitude	0 17 57·19

" And, lastly, we received from Cambridge a single tap for the purpose of comparing clocks, and the result deducible from it is exhibited in

Table IX.

	Clock Time. h m s	Clock Error +. m s	True Sidereal Time. h m s
At Fredericton	8 7 21	3 23·7	8 3 57·3
Cambridge	7 46 48	0 47·89	7 46 0·11
		Hence the difference of longitude	0 17 57·19

" On examining the operations of February 10, it will be perceived that the second-beats of the Boston chronometer and the Fredericton transit-clock continued synchronous throughout, and, therefore, must have had the same rate. Hence the same clock-errors are applicable to the whole of the series for the evening. We may remark, that the results obtained from this last night's work are considered the most complete and satisfactory, and from them alone the difference of longitude would be $0^h\ 17^m\ 57^s{\cdot}18$. If, however, we take the mean of all the operations, the difference would be $0^h\ 17^m\ 57^s{\cdot}23$; and as Cambridge Observatory is $4^h\ 44^m\ 30^s{\cdot}66$ west of Greenwich, it follows that the longitude of Fredericton is $4^h\ 26^m\ 33^s{\cdot}43$ west of Greenwich. Converting the above time into arc, we have,—

	° ′ ″
Longitude of Fredericton	66 38 21·5
The Crown Land Department makes it	66 37 54
Difference	0 0 27·5

" This difference is smaller than could have been anticipated, or than we should have been warranted in assuming.

" J. B. Toldervy.
" W. B. Jack.

" *Fredericton, March* 5, 1855."

The following is an extract of a letter from Professor Jack to the Astronomer Royal, dated Fredericton, April 19, 1855, in reference to the foregoing determination:—

" Morse's machine was employed, and as the armature of the magnet at one end of the line has, on the connexion being made

by means of the finger-key at the other, to move through a space not probably exceeding the $\frac{1}{200}$ part of an inch, and as the contact of the armature and magnet gives a distinct sound, which can be easily compared with the second-beats of the transit-clock, we cannot but think this method susceptible of greater accuracy than that which depends on watching the movements of a needle. Still greater accuracy could have been obtained had we been supplied with Bond's self-registering apparatus."

The following is an extract of a letter, dated April 23, 1855, received from George Hamilton, Esq. F.R.A.S., Egremont, near Liverpool, in reference to Mr. Elliott's invention of a Mechanical Imitation of Precession, alluded to in the last number of the *Monthly Notices:*—

"In the year 1842, Mr. Edward Davis, optician, then of Liverpool, now, I believe, of Shrewsbury, constructed for me an apparatus precisely the same in principle as Atkinson's, and precisely the same in form as Elliot's. I have used the apparatus since that time to illustrate precession of the equinox and nutation of the earth's axis. My apparatus was made after a model shown to me by Mr. Elliot, and, judging from the appearance of his apparatus, I should conclude that it had been constructed many years before."

Elements of Leucothea. By M. Bruhns.

Epoch, 1855. May 0·0, M.T. Berlin.

M	= 351° 16′ 22″·7	
ϖ	= 212 55 38·7	Mean equinox, 1855·0
Ω	= 355 42 15·2	
i	= 7 37 35·9	
φ	= 14 17 36·9	
μ	= 694″·479	
log a	= 0·472232	

These elements were calculated from an observation made at Bilk, dated April 20; and two Berlin observations of April 27 and May 3.

Note relative to a Phenomenon seen in the Planet Venus. By the Rev. W. R. Dawes.

"In the *Monthly Notices* for March 1854, there is a notice of some observations of the planet *Venus*, when near her inferior conjunction, by Mr. Guthrie. The phenomenon he observed is

very interesting; and it seems desirable that some more definite statement should be furnished by Mr. Guthrie respecting the observations, giving the exact *date*, the approximate *altitude* of the planet, or the *time* of observation from which the altitude and the degree of twilight might be computed.

"It is to be regretted that a notice of the observations was not published at the time, as it might have led to a repetition of them by other observers on subsequent opportunities.

"I confess that I have frequently examined *Venus* near her inferior conjunction, with the view of ascertaining if any such phenomenon were visible, as her apparently dense atmosphere might seem to render probable, but have never caught sight of anything which could be supposed to arise from that cause, except a very moderate elongation of the extremities of the crescent.

"*Wateringbury, April* 30, 1855."

[At the time when Mr. Guthrie's Note was communicated to the Society the Editor endeavoured to obtain some precise details respecting the phenomenon to which it refers, but it was found that Mr. Guthrie did not possess any written record of his observations. He merely remembered that they were made on the occasion of the inferior conjunction of the planet in December 1842. This was not, indeed, explicitly stated by him, but was inferred from the terms of his letter.]

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*Extract from a Paper by Eyre B. Powell, Esq. on the Orbit of α Centauri.** (See *Monthly Notices* for January last, p. 87.)

"I have seen no other orbit of *α Centauri* than the one calculated by Captain Jacob, and appended to the Poonah Catalogue of Double Stars: between his and the present there is a great resemblance as to all the elements, save γ, Ω, and a, but in these we differ *in toto*. A table of variations, which I computed, explains our discrepancies: in the angles of position, like variations of γ and Ω, produce opposite changes, such that, from 1826 to 1848, a decrease of the inclination can be to a very great extent compensated by a retrogradation of the line of nodes; in the distances also there is a similar adjustability among different elements, the variations of a, γ, and Ω, playing the most prominent parts. The evident proximity of the primary star to the following side of the projected ellipse, and the necessary area of the perspective orbit, seem to mark that the line of nodes must lie nearly in the meridian, and if so, γ must possess a large value. With my second set of elements the area of the projected ellipse

* These remarks would have been inserted in the *Monthly Notices* for April, along with those of Captain Jacob on the same subject, had not the Paper which contained them been at the time in the hands of the printer for publication in the volume of Memoirs.—Ed.

comes out 154 square seconds, and the sector between Captain Jacob's observations of 1846 and 1854, taken in connexion with a period of 75·3 years, gives an area of 153·6 square seconds for the ellipse. The elements also represent the observation of Fallows, as to position, with an error of only $+44'$, and that of La Caille, as to distance, with the trifling error of $-''\cdot34$."

Notes on the Management of Chronometers and the Measurement of Meridian Distances. By Captain Charles F. A. Shadwell, R.N. C.B. &c. London, 8vo. 1855.

The author states that this work is mainly designed for the use of officers of the Royal Navy, but he expresses a hope that it may be perused with advantage by intelligent individuals of the Mercantile Marine. His object has been not so much to show how Chronometers may be rendered subservient to the ordinary purposes of Navigation as to exhibit their application to the advancement of Maritime Geography.

The work is divided into nine chapters, the first three of which are devoted to various preliminary details relative to the management of chronometers and to the determination of local time. In the fourth chapter the author explains the mode of ascertaining the rates of chronometers by two observations of a similar kind taken at a convenient interval. On this part of his subject he remarks :—"For the determination of the error on local mean time, it has formerly generally been the practice in the measurement of meridian distances, or on taking a departure from a port, to adopt as the starting-point the error shown by the last observation used in the determination of the rate; but inasmuch as in the deduction of the latter, we necessarily place equal confidence in both the observations, there seems to be no good reason why we should not do the same in assuming the error and adapting as our *working* error the mean of the two errors on which we have already agreed to make the rate depend.

"The *mean* error thus introduced may probably be assumed to be more accurate than either of the single elements on which it depends; while this mode of proceeding, moreover, will have the advantage of referring both the error and the rate to the same epoch."

The author remarks that this mode of viewing the subject has the advantage of simplifying the process for finding the corrections to be applied for the changes of rate which may have taken place in the transit from port to port.

The principles explained in this chapter are illustrated by a copious collection of examples. In one of these the error and rate of the chronometer are determined from observations of a *dissimilar* kind made in two places whose *difference of meridian is known.* "This example," says the author, "affords an illustration of an important remark of Raper's, 'that as the longitudes of the

several places approach to precision, ships will employ the difference of longitude as a means of obtaining directly the *sea-rates* of their chronometers, instead of waiting to obtain harbour-rates; thus exemplifying one of the most important ends to which the perfection of hydrography can serve.' "

Upon the question with respect to the best interval for determining the rates of chronometers, the author justly remarks that if the stability of the rate could be relied upon, its value would be obtained with greater accuracy the longer the interval of time included between the observations for ascertaining the values of the error on mean time.

" In practice, however, this theoretic view is limited in its application by the impossibility of depending confidently on the steadiness of the rate over long periods, and by the consequent necessity for checking the performances of chronometers by frequent determinations of their errors, and thus breaking up the intervals on which the rates depend into short periods.

" As a matter of practice, therefore, it seems advisable when circumstances permit, that the rate of a chronometer should not depend on observations made at an interval of *less* than *five* or *more* than *ten* days. Seven days will be found a convenient average interval; and in the case of eight-day chronometers, moreover, it embraces the period affected by the whole weight of the chain.

" With the above limitations it may be laid down as a maxim that chronometers cannot be rated too often when time and opportunity permit.

" It seems advisable, moreover, when the measurement of meridian distances is in contemplation, that in so far as may be practicable, the two rates employed should depend on observations made at *equal* intervals of time; since, when the intervals are very unequal, the small errors of observation do not exercise an equal influence on the final results, and their values are unduly affected by the errors of observation attendant on the rate determined at the shorter of the two periods."

In the fifth chapter the author explains the mode of obtaining the errors and rates of chronometers by a combination of several observations taken within a convenient interval of time. The accumulations of the rate for the partial intervals form the groundwork of a series of equations of condition, which are solved by the method of least squares. On the question with respect to the advantage attending the application of this method, the author remarks, that " although in most cases the mean arithmetic error corresponding to the mean of the times of observation may be considered as sufficiently accurate and convenient for practice; yet in cases where an examination of the observations seems to indicate considerable instability or fluctuation of rate during the period of rating, and when the computer does not object to the additional labour involved in the latter more elaborate process, there is no doubt its results will usually be more satisfactory, and

certainly more correct, while it will fully repay the extra trouble employed in its manipulation."

In the sixth chapter the author treats of the chronometric determination of meridian distances, explaining the different hypotheses which have been adopted for finding the variation of the rate during the voyage between the two places where the rate has been ascertained. The following chapter contains a copious collection of examples illustrative of the principles laid down in this chapter.

In the eighth chapter, the author shows how the difference of longitude between two places may be obtained from observations giving the *errors* of the chronometers at the two stations, independently of a knowledge of their preceding or subsequent *rates*. The *error* of the chronometer is ascertained: first, upon starting from the first station; secondly, upon arriving at the second station; thirdly, upon quitting the second station; and, lastly, upon arriving again at the first station. From these data the difference of longitude between the two stations, and the rate of the chronometer for the interval included between the departure from and subsequent arrival at the first station, termed the *travelling rate*, is deduced by a formula for which the author expresses his obligation to the Rev. George Fisher.

The ninth chapter of the work is devoted to a series of precepts on the mode of recording the results of chronometric measurements.

It may be remarked, in conclusion, that the subject-matter of the work is arranged in a very lucid order, that the explanations are concise and simple, and that they are in every instance illustrated by numerous examples. Possessing such claims to consideration, the work cannot fail to prove a most valuable manual to the naval profession in general, and more especially to the class of persons whose instruction the author had mainly in view while engaged in preparing it for publication.

Captain Shea has forwarded for inspection a volume containing the record of his observations of the solar spots. The appearances and disappearances to which he refers seem to be no other than the ordinary phenomena depending on the rotation of the sun upon its axis.

An azimuth compass for determining the variation was exhibited by Baron Kleinsorgen. It is assumed that the time of noon is known or may be determined on board, and consequently the direction of the true meridian at that moment. A line is stretched across the compass-box, and this box is turned until the shadow of the line coincides with the diameter of a fixed plane a little distance below it. The pin on which the card turns is

connected with this plane, and there is an index corresponding with the diameter which points out the reading of the compass for the time of observation. Possibly in those merchant-ships where the altitude of the sun at noon is the only observation made, such an instrument might give useful warning to the helmsman, but the method only admits of a loose approximation.

New Variable Star.

Professor Argelander, in a letter to the Editor of the *Astronomische Nachrichten* (*Astron. Nach.*, No. 9580), announces the discovery of a new variable star, namely, the star W. VI. No. 1911 = Lal. No. 13825. It is represented by Lalande as an 8^m; by Bessel, once as an 8^m, and once as an 8.9^m. Fellöcker marks it in the Catalogue as an 8^m, but upon the Chart as an 8.9^m. Bremiker makes it a 9^m. However, on the 4th and 5th of March, 1854, and on the 4th of March, 1855, it was not visible in the comet-searcher, nor was it seen in the heliometer on the 30th of the same month; but it must be admitted that on this last occasion the sky was unfavourable for observation. Two days afterwards, however, it appeared like a faint star of the 9.10^m; and on the 11th and 12th of April it shone like a faint 9^m. It is of a very ruddy colour, and, like many other variable stars of small magnitude, appears to have a period of rather more than a year. Professor Argelander proposes to call it *R Canis Minoris*, and expresses his intention of following it as long as possible.

On several Stars which have Disappeared from his Ecliptical Charts. By M. Chacornac.

"On the 7th of August, 1852, at 15^h, I placed upon one of my charts, in $21^h\ 36^m{\cdot}5$ of right ascension and $+\ 14^\circ\ 33'{\cdot}9$ of declination, a star of the seventh magnitude, which was situate between two stars of the ninth magnitude. The position of this star was obtained by comparisons made with another star of the eighth magnitude in its vicinity.

"Next day I only compared with the heavens the regions of the chart where small stars were found to be situate; unfortunately, the region in which the star of the seventh magnitude had been placed was almost entirely wanting in small stars. I did not, therefore, verify it on that day, expecting on a future occasion to fill it up with small stars, and to re-observe at the same time the star which I had placed there.

"On the 20th of the same month, having finished this chart, I was very much surprised not to find again the star of the seventh magnitude, while those of the ninth magnitude were found to be perfectly in the place which my chart assigned to

them. Convinced by this circumstance, and by the verification of the numerical results obtained on the 7th of August, that there could be no mistake, I undertook forthwith the search of this star upon the supposition that it was a planet, describing its arc of retrogradation. With this object in view, I constructed a rough draught of the neighbouring stars as far as the ninth magnitude. On the 30th of August this chart already comprised in the direction of the retrogradation 14° of right ascension, and extended on each side of the ecliptic 8° to 10° of latitude, without obtaining any result. I then learned that Mr. Hind had just discovered the planet *Melpomene* in the very region which I was exploring. This planet being fainter than the stars with which I was occupied, I immediately abandoned the search of the star of the seventh magnitude, which I supposed to be variable, in order to resume the construction of my charts. Since that time the star has not reappeared.

"On the 30th of December, 1852, I placed upon my charts, in $8^h\ 47^m{\cdot}3$ of right ascension and $+17°\ 44'$ of declination, a star of the ninth magnitude. I verified the position of this star only twelve months afterwards; that is to say, on the 4th of December, 1853. It was then invisible, and has not since reappeared. This star was near another very red star of the sixth magnitude.

"On the 5th of July, 1853, I also placed on a rough draught, in $16^h\ 8^m{\cdot}8$ of right ascension and $-22°\ 21'$ of declination, a star of the ninth magnitude near a nebula; these stars were not verified in the same year.

"On the 19th of May, 1854, upon completing this chart, I noticed that this star of the ninth magnitude had disappeared from the place where it was observed. It remained invisible till the month of August of the same year. Being sought for again on the 4th of April, 1855, it was found to be of the tenth magnitude.

"On the 8th of April, 1853, I observed a star of the eleventh magnitude in $11^h\ 3^m{\cdot}3$ of right ascension and $+6°\ 54'$ of declination. This star had precisely the same right ascension as another star of the ninth magnitude which was very near to it. On the 15th of the same month it was invisible. Having been again searched for on the 16th and 17th as a planet, I was not able to find it, either on account of the brightness of the moon or in consequence of its inconsiderable motion. On the 18th I knew that it was the planet *Themis* which M. De Gasparis had just discovered.

"On the 27th of December, 1853, I observed between two stars, very close to each other, between the eighth and ninth magnitudes, a star of the tenth, which I had not hitherto noticed. This chart not being filled up with small stars, I did not attach any other importance to the observation than that suggested by a multitude of new stars which are encountered in observations of this kind. I, therefore, placed this star of the tenth magnitude upon my chart, writing beside it at full length *triple star*.

"This triple star was only verified on the 26th of March, 1854.

It was then only double, the small star of the tenth magnitude having disappeared. I examined this double star with a high magnifying power, and I again searched for the star which had disappeared upon the supposition of its being a planet, but without result. The following is the position of the double star:—$4^h\ 14^m{\cdot}6$ of right ascension $+23^\circ\ 58'$ of declination.

"On the 30th of December, 1853, I observed a star of the eleventh magnitude in $3^h\ 33^m{\cdot}7$ of right ascension and $+20^\circ\ 51'$ of declination. The unfavourable state of the weather prevented me from verifying its position with certainty till the 20th of January, 1854; it was then invisible. I have searched for it again as a planet, but without success. It has not reappeared.

"On the 10th of January, 1854, I found that a star of the eleventh magnitude, which must have been placed upon my charts between the 4th of September and the 29th of November, 1853, in $4^h\ 26^m{\cdot}9$ of right ascension and $+21^\circ\ 26'{\cdot}8$ of declination, had disappeared. I searched for it again in my charts as a planet without result. It has not reappeared.

"On the 26th of January, 1854, I undertook the construction of a rough draught, upon which I placed very few stars during the evenings of the 30th and 31st of January. I did not resume the construction of this chart till the 30th of July of the same year: two stars had then disappeared from it. The one of the eleventh magnitude was not reobserved till later with the great refractor of the Observatory of Paris; it was then only of the thirteenth magnitude. The other of the eleventh magnitude has not been reobserved. The latter was situated in $23^h\ 27^m{\cdot}5$ of right ascension and $-4^\circ\ 15'$ of declination.

"On the 19th of July a star of the tenth magnitude was placed upon my charts in $21^h\ 7^m$ of right ascension and $-15^\circ\ 5'$ of declination. It was the planet *Urania* found by Mr. Hind. On the same day I found that a star of the ninth magnitude which must have been placed upon the chart in the course of the month of January 1854, in $21^h\ 20^m{\cdot}2$ of right ascension and $-12^\circ\ 53'$ of declination, had disappeared.

"This star, which had been observed during the twilight, formed with other stars of the ninth magnitude a remarkable configuration. It was a little more brilliant than another star also of the ninth magnitude, which was very near to it.

"On the 11th of January, 1855, I found that a star of the eleventh magnitude, which had been placed on the 26th of October, 1854, upon one of my charts in $7^h\ 30^m{\cdot}3$ of right ascension and $+23^\circ\ 54'{\cdot}7$ of declination, had reappeared. I was not able to reobserve this star earlier on account of the unfavourable weather which prevailed at Paris during the whole season of autumn. It has not reappeared.

"On the 28th of October, 1854, I found that a star of the eleventh magnitude, placed upon my charts in $2^h\ 47^m$ of right ascension and $+17^\circ\ 32'{\cdot}5$ of declination, had disappeared on the 26th of September in the same year.

"It was a position of the planet *Pomona*, which M. Goldschmidt had just discovered.

"In the ninth hour, and quite recently, two stars have also disappeared from the positions which my charts assigned to them.

"One of the tenth magnitude which was placed upon these charts towards the end of the year 1854, has been invisible since the 17th of January, 1855. The other, which was of the eleventh magnitude, was observed on the 25th of January of the same year, and placed upon my charts in $9^h\ 27^m{\cdot}2$ of right ascension and $+16^\circ\ 8'{\cdot}7$ of declination. It had disappeared on the 19th of March, at which date only I was enabled to verify its position.

"These two stars have been searched for again as planets and variable stars, but without result.

"On the 5th of October, 1853, I perceived a star of the twelfth magnitude in a region wherein my chart did not exhibit any. This chart was, however, pretty well filled up. I affixed a mark to this star and proceeded on the following day to verify its position. Clouds permitted only a short verification, during which I found that the star had disappeared from the place where I had observed it on the preceding evening, but I was unable to find it again in the neighbouring region. I searched for it again on the following days with great care, but without success. Since then it has not been found in the position of the 5th of October, which was this,—

R.A. $= 0^h\ 44^m{\cdot}4$ Decl. $= +8^\circ\ 46'{\cdot}2$

"In order that it may be understood how I have allowed so many stars to escape from me, I ought to mention that, in order to construct my ecliptic zones with rapidity,—although I always had the intention of verifying on the following evening the position of the stars observed on any preceding evening,—I have frequently been compelled by the unfavourable change in the weather to defer the verifications to other epochs, and to profit by the beautiful nights in occupying myself especially with the regions of the ecliptic in opposition to the sun. I ought also to mention that it has happened to me, at Paris especially, to have been obliged to postpone for several months similar verifications by the coincidence of the rare instances of a clear sky with the full moon; for it is known that when the question relates to the small planets, the great brightness which the moon diffuses over the entire heavens, when it is approaching opposition, effaces their light as effectually as the close proximity of that body. The usually not very serene condition of the sky at Paris, the inconsiderable altitude of these stars above the horizon, are so many causes which prevent the searching for and verifying of them as often as is possible in the case of stars of a higher magnitude.

"*Imperial Observatory, Paris, April* 15, 1855."

Astronomical and Meteorological Observations made at the Radcliffe Observatory in the Year 1853. Under the Superintendence of Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XIV. 8vo. Oxford, 1855.

The principal part of this volume is devoted to the class of observations required for the construction of the Circumpolar Catalogue. An appendix to the volume contains the observations with the heliometer. In the introduction to this part of the work Mr. Johnson gives the details of his investigation of the parallaxes of 61 *Cygni* and 1830 *Groombridge*, to which allusion was made in the Annual Report of the Council for this year. With respect to the observations with the heliometer, a few consist of observations of the diameters of the planets; but they chiefly refer to double stars, and to measurements of the relative positions of a few select stars for parallax. The latter class of stars includes *Arcturus* and *α Lyræ*.

New Comet.

A comet was discovered by M. Klinkerfues at Göttingen on the 4th instant. The following position of it was obtained by M. Rümker at Hamburgh on the next evening:—

	Hamburgh M.T.	Comet's R.A.	Comet's Decl.
	h m s	° ′ ″	° ′ ″
1855, June 5	10 49 37·0	107 50 58·7	+36 15 53·5

Instruments for Sale, formerly belonging to the late Bryan Donkin, Esq.

A $3\frac{1}{2}$ in. achromatic telescope and claw-stand, by Tulley	£35 0 0
A position micrometer, by Troughton	4 0 0
A mountain barometer, by Troughton	7 0 0
Ditto, by Bautern	3 0 0
A 30-in. transit and stand collimator, and a Donkin level for ditto	15 0 0
A sextant and stand, with bottle, &c., by Troughton ..	14 0 0
A 12-in. theodolite, plates and legs, by Troughton ..	10 0 0
A Y level, 30 inches, by Simms	6 0 0
A level, 20 inches and legs	5 0 0
A dynameter, by Dollond	2 0 0
Two 2-feet pentagraphs, one by Troughton	3 0 0
A small solar microscope	1 0 0
A perspective machine	1 0 0
An equatoreal block	1 0 0
A grinding tool, by Holtzapffel	1 0 0
A hygrometer, by Daniel	1 0 0

They may be seen at the rooms of the Royal Astronomical Society.

Instrument for Sale.

Object-glass, by Lerebours and Secretan of Paris; focal length, 86·3 in.; aperture, 6·2 in.; not quite perfect, having striæ at two points of the circumference. The central four inches are very good. The measures of 144 double stars given in the last volume of *Madras Observations* were taken with this glass. Price 28*l.* Inquire at the apartments of the Society.

A Lassell's machine for grinding and polishing specula for reflecting telescopes, made by Mr. Nasmyth of Patricroft. Price 30*l.* Inquire at the Society's apartments.

ERRATA.

The upper line of the figure X, referred to by Captain Jacob in his Paper on the Orbit of *α Centauri*, inserted in the *Monthly Notices* for April (p. 179), instead of being *rectilinear*, as incorrectly drawn by the engraver, should have been *slightly curvilinear*, so as to adapt itself to the periphery of the ellipse.

Page 181, line 4 from bottom, *for* 52° 24′ 52″, *read* 52° 54′ 52″.

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